

Interactive comment on “Apparent oxygen utilization rates calculated from tritium and helium-3 profiles at the Bermuda Atlantic Time-series Study site” by R. H. R. Stanley et al.

Anonymous Referee #1

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This short paper aims to measure respiration rates in the thermocline using observational data. The authors determine water mass ages combining tritium-helium data and TTD. Uncertainties are discussed in greater detail in section 4.

The overall approach is not very new, but the paper summarizes the methodology very well, and is a well-written manuscript with excellent scientific clarity. Having said this, I have a number of comments as listed below. I encourage the authors to revise the paper before the publication of this paper.

(Major comments)

First, the conclusion that the AOUR could have increased over the last several decades
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is flawed as the authors themselves point out the problem with the old oxygen data. There is no substantial reason to speculate that respiration has increased by such a large amount. One should not draw a conclusion based on highly uncertain data.

Secondly, I feel that the introductory paragraph needs a revision as it trivializes observed oxygen changes as a simple manifestation of global ocean de-oxygenation. Authors make a reference to [Deutsch et al., 2011] on page 9978 line 16 in the context of de-oxygenation. However, what Deutsch et al discusses in that paper is more complex than just a monotonic trend. Rather, the size of tropical Pacific Oxygen Minimum Zone is correlated with the Pacific Decadal Oscillation through the reinforcing changes of upwelling and oxygen utilization in the thermocline.

Third, I really like the discussion in page 9980 line 3-6. It is very important that the role of horizontal ventilation at the water parcel can pass through different biological regimes. Therefore, the data presented in this paper is not relevant to the vertical profile of sinking particles. I don't see a point in section 4.1.

Fourth, “relatively small” is not very convincing on page 9983 Line 4. It would be reassuring to see that spatial variation of surface TU is indeed small (or not?) relative to the signals of interest. Some of the discussion in section 4 should be moved up here.

(Technical issues)

Page 9984, line 4, γ/δ is analogous to the square root of the Peclet number (Pe). For the one-dimensional advection-diffusion equation with constant coefficients, the ratio is exactly equal to the square root of Pe.

Page 9984, line 7 is an incomplete sentence. Please be explicit about the assumption between γ and δ .

Page 9984 line 21. Please clarify that the best fit is still subject to a specific choice of γ/δ relation. This could be a part of uncertainty analysis.

Page 9988 line 4, please remove “a”.

Page 9990 line 25. It does not make sense for the isopycnal trajectory to “decrease”.
Do authors mean “deepens”(i.e. move downward) instead of “decreases”?

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